botany seems to be having on my estimable colleague. Some zoölogists divide the organs of animals into the vegetative organs, their functions being those common to plants and animals, and the organs of animal function, their functions being characteristic of animals. My genial associate must have learned this fact from some one and makes a desperate effort to use it in classifying the sub-sciences of biology by trying to limit zoölogy to the vegetative organs of animals, and relegating the animal functions to psychology, which is held "coördinate with zoölogy rather than as one of its sub-divisions." I may be wrong, but that effort looks like a bid for a vote.

To guard against any misapprehension on the part of those not acquainted with the actual attitude of the departments of botany and animal biology toward each other at the University of Minnesota, I must say that Professor MacMillan and myself are not at loggerheads here, but that we do and always have pulled together for the equal advancement of both botany and animal biology. The adjustment of our courses is not the result of a compromise, but the individual and united recognition of facts and conditions. We are not competitors, and there is no likelihood that we shall become such.

HENRY F. NACHTRIEB.
Professor of Animal Biology, University of Minnesota.

April 18.

On Methods of Defending the Existence of a Sham Biology in America.

TWO recent papers in *Science* deserve a little attention at this time, for they serve as examples of the kaleidoscopic movements by which "biologists" hope to defend themselves against the clearly stated charges of incorrect use of terminology which have been brought against them. It will not be permitted to these wanderers from the path of orthographic rectitude to conceal their retreat under cover of a sea of ink. The discretion, good taste, enthusiasm of the writer, are not the subjects of the discussion and will not be discussed by him. No shuffling to alien positions can be admitted as an answer to the definite impeachment which has been brought against courses in zoölogy masquerading under the erroneous name of biology.

Although the briefer, the article by Mr. H. F. Osborn [1] of Columbia College should, from the acknowledged ability of its writer and its air of gentlemanly candor, be given first consideration. Mr. Osborn is under such manifest misapprehension, however, that it will be necessary first of all to correct him and indicate to him just the point at issue. He says "the arrangement of courses in Columbia is cited by Mr. MacMillan as a leading example of the manner in which botany is subordinated to zoölogy." Since absolutely nothing was said in my article about the subordination of botany to zoölogy at Columbia or anywhere else, I am naturally interested to learn by what higher criticism, textual or literary, Mr. Osborn has arrived at such an unexpected result. In my former paper it is written, "At Columbia College it is apparent that the subject of botany, since it stands by itself under its own organization, is supposed at least by the 'biologists' of that institution to be quite without the pale of their own science." It is my evident and distinct purpose here to charge, not subordination, but misuse of terminology. Indeed, if there were any "subordination" at Columbia, I should think it would be of the zoölogical courses staggering as they are under the weight of a false nomenclature.

In his note, Mr. Osborn cites a number of botanical and zoölogical courses at Columbia and then uses the word "biological" correctly in the sentence, "It does not appear that botany is ignored in this programme of *biological* courses in this institution." Immediately afterward he uses the word incorrectly when he says, "the fact that the botanical courses are not arranged under the *Biological* Department is a mere technicality of administration." A "Biological Department" without botanical courses is, however, something more than a "technicality"; it is a sham. Mr. Osborn is, of course, at liberty to have his department separated as he will; it is no affair of ours, — but why should he permit such a line as this from the circular of information, [2] "Biology (Zoölogy) . . . Professor Osborn"? Why does he appear as defining the word biology as zoölogy? I am sure it must be for some better reason than the anxiety to use a high-sounding word, even though that word be used incorrectly.

Having thus indicated to Mr. Osborn the errors into which a probably hasty perusal of my former article has led him, I may now note his principal defensive movement. He says, "Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life." Therefore, "those who set forth the fundamental principles of life are biologists,"—a fair paraphrase, I trust, of Mr. Osborn's argument. This is so unexpected a point of view to be taken by one of the leading animal morphologists of America that it is indeed difficult to collect one's self for a reply. The venerable style of talk about "life," I supposed, was extinct in scientific circles, unless one includes the metaphysicians. "Life," I had supposed, was an abstraction from certain observed phenomena of a group of things known as plants and animals. I presume Mr. Osborn does not use the word as does the Boston University in its Year Book, [3] where Group IX. in Courses of Instruction is "Chemistry, *Biology*, and Geology," and Group X. is "*Life*, Personal Development, and Expression." I did not suppose that the statement that "biology was the science of living things" could possibly find objection in such a quarter as Columbia College. Here at Minnesota we are busily studying living things, but if Mr. Osborn is studying "life," he is evidently on another plane altogether. Long ago, one used to hear of "vital force" and "life," but I supposed we now believed that the best way to learn about life was to study living things. If it is true that the zoölogists are going in for the study of "life" under the belief that biology is not the science of living things, I wish them God-speed on a perilous, if ancient, voyage. And if this really is the modern view of "biology," I yield me a captive to Mr. Osborn's convincing argument and beg to withdraw among those botanists who believe that botany is the science of the living things, plants, and will certainly, if I know them, be glad to leave the study of "life" open to the zoölogist-"biologist," who rules out living things as irrelevant to his science.

Let me, in closing, call the attention of Mr. Osborn to the fact that I am unaware of any one-sided state of true biological education in America. There is nothing one-sided about it in Harvard University. It is the sham biology that is one-sided, and for this the zoölogists are responsible in large measure, therefore the epistle is addressed to them. I recall now but one institution which names its botanical courses, a "department of biology." And this department is manned by a Johns Hopkins doctor of philosophy, from whom one might unfortunately expect the one-sided view.

The paper by Mr. Francis H. Herrick, [4] entitled "On the Teaching of Biology," requires some elucidation and correction that I may venture to give. Notwithstanding its characterization of my former article as "thoroughly bad," I take pleasure in acknowledging its own uncommon excellence. Any defense of the sham biology is sufficiently difficult, and while the air of righteous enthusiasm was accurately enough predicted it was scarcely realized with what vigor the plaintiff's attorney would be afforded the treatment sanctioned in such cases by all the traditions of the bar.

Aside from its entertaining personal character, the contribution by Mr. Herrick appears to seek the establishment of the following points: (1) The study of biology is not two disciplines, but one discipline; (2) biological science is not to be set over against physical science, but is to be included in it; (3) zoölogy, when presented under the name of biology, is not a sham biology, but a "restricted biology"; (4) the better fundamental division of biology is into general morphology and general physiology, not into botany and zoölogy. Stated thus, with such condensation as is necessary for clearness, it is hoped that the exact meaning of Mr. Herrick is preserved. These four points, only the third of which seems to have direct bearing on the question at issue, may now receive their proper attention.

[1] Science, Vol. XXI., p, 234. New York.
[2] Columbia College Circular of Information, 1893, Pt. VII., p. 4. New York.
[3] Boston University Year Book, Vol. XX., p. 66, 1893. Boston.
[4] Science, Vol. XXI., p. 220. New York.

(1) There is certainly a unity in the science of biology. This unity is not, however, zoölogy. Breadth of view demands rather a recognition of the true unity, and for such recognition the writer is contending. "Good observation" will convince Mr. Herrick that one who writes "Biology is either a superficial smattering of natural history facts and methods — and in this case not of any value — or a strong, uniform presentation of the facts of botany and zoölogy — and in this case a very different thing from a sham biology which is principally, or all, zoölogy" —doubtless appreciates the breadth of biological science almost, if not quite, as clearly as he would if contending that zoölogy alone may pass current for biology. For such higher unity of biology it is a duty to contend against any or all disintegrating views that may arise from the misfortune of a narrow education.

(2) Mr. Herrick laments the inadequacy of my early training along biological lines and, indeed, charges me in so many words with having been myself a student at Johns Hopkins University. As principal evidence of an indwelling incapacity he adduces my setting biological science over against physical science. He writes, regretfully reminiscent: "a student who had followed this general biological course with a fair degree of success, would have learned that 'biological science is not to be set over against physical science in the broadest sense,' but that in this broadest sense biology is a physical science coördinate with chemistry and physics." In this connection the following quotation may be noted. It is from Dr. C. O. Whitman, an acknowledged leader, I believe, in American zoölogy: "The term biology is so frequently used with latitudinarian disregard of its etymological significance that it becomes necessary to recall its original meaning. . . . As still used by the best authorities, the term is a very comprehensive one, denoting not one science or the fragment of a science, but a multitude of sciences embracing the entire organic world in contradistinction to the inorganic or physical world. From this broad standpoint all the natural sciences fall into two great groups, known as the biological and the physical."[1] Doubtless, no italicizing will be required to impress Mr. Herrick with the ripeness of the harvest that awaits his discriminating mission-effort among his biological colleagues. With his fine solicitude for those whose "early comprehensive training" has not sufficed to distinguish clearly between physical and biological science, he will scarcely permit himself to overlook so distressing a failure in Dr. Whitman to conform with the standard of absolute correctness. The writer, however, must continue to believe that a grouping of natural sciences into physical and biological sciences is not altogether unproductive of right thinking and ventures to commend, as a useful discipline, to Mr. Herrick, the reading of Dr. Whitman's programme not only on account of the value of its definition of biology and the general breadth of its views, but also because a uniform line of defence will be highly advantageous for all who find themselves, whether by necessity or by choice, enlisted under the flag of the sham biology.

(3) While the term "restricted biology" is an ingenious suggestion for such courses in zoölogy as are offered at Columbia College and Johns Hopkins University under the inappropriate name of biology, it is not clear that the old-established word "zoölogy" is not better. It is scarcely so vague and has the merit of brevity. If either of these institutions should gracefully announce a "department of Restricted Biology" and should confer degrees upon "doctors of philosophy in restricted biology" it would certainly indicate the dawn of ethical development if not the noon-tide of philological precision. And if such a consummation lies near the heart of Mr. Herrick he shall not wander farther without my sympathy. But, unfortunately, one must here note the crucial and deplorable fact; these institutions do not employ the term "restricted biology," but use instead the broader term, biology, for their zoölogical courses. Since a part of anything posing valiantly as the whole is universally recognized as a sham, it is hardly possible in such a case for the sham biology to escape its just characterization.

(4) It is unreasonable, of course, to ask that an American "biologist" should be familiar with the literature of plant-morphology from Hofmeister to Guignard, Strasburger and Treub. But those humbler botanists that have followed the progress of recent investigation in this field would realize how distant seems to be the day when general homologies between higher plants and higher animals may be demonstrated with certainty. The established fact that between sporophytic plant-embryos and gamozoan animal-embryos there exist few known homologies-in-general must give pause to ambitious talk about a "general morphology." Such a general morphology would certainly demand a basis of general phylogenetic and ontogenetic comparison. It is true that in cytology, and especially in nuclear dynamics there may be read, for the future, possibilities of a general morphology. Chromatomeres may indeed be always homologous as well as analogous, broadly speaking. But to-day "general morphology," as a science, does not exist. I should be glad to learn the title of some compendium of general morphology. I should be pleased to hear the name of some living or deceased investigator who could, in the broad sense of Mr. Herrick's division, be termed a "general morphologist." The fundamental division of biology into two sub-sciences, one of which, at least, does not exist as such, seems scarcely so productive of good as the time-honored division into botany and zoölogy. Between plant-physiology and plant-morphology there are innumerable series of contact-points. Between plant-morphology and animal-morphology there are few. Until, therefore, we may claim a far wider knowledge of the facts of morphology and physiology—at least in the field of botany — it will be difficult for Mr. Herrick to impose his divisions of biology to suit the terms of his argument.

In the second place, the science of biology is clearly not principally a method or discipline as Mr. Herrick seems to think it is; it is, also, and primarily, an orderly group of facts about an orderly group of things. These things are living things. The primary division must therefore be along the line of mass, not along the line of method. Living things conveniently divide with great exactness — although not absolutely, as Mr. Herrick acutely indicates — into plants and animals. Biology, therefore, divides conveniently into botany and zoölogy. A particular method is the essence of morphology, but plants-in-the-aggregate are the essence of botany. Biology is, primarily, a group of facts about a group of things, not a group of facts about a group of methods of studying things. Plants (for example) are things, not methods, and therefore the fundamental division of biology into botany and zoölogy is more logical than its division into morphology and physiology. It thus appears not only that the divisions of biology urged by Mr. Herrick have never existed and do not exist now, but also that logically they should not exist as primary divisions but only as secondary. Finally, even if they did and should exist, the classification would not help the sham biology. For the union of a sham "general morphology" and a sham "general physiology" would probably result in a sham biology, and a "general morphology" which upon criticism reveals itself as the special morphology of animals is evidently a sham morphology.

It is a source of regret to the writer that anyone should suppose that he would "stigmatize" any university or any honorable graduate of a university. His function is purely indicative, and, while he agrees with Mr. Herrick that the truth about the state of affairs in certain curricula and the state of culture in certain graduates is so melancholy that perhaps even so strong a word as "offensive" may rightly be applied, he must disclaim any connection with such a condition beyond that of an interested spectator, grieved that able young men should be dwarfed in their conceptions of the great field of biology through acceptance of a sham in place of the truth. He has the kindest of feelings for such young men and a warm sympathy for institutions straining every nerve in an unequal struggle with others of greater wealth and breadth. But he cannot permit his sympathy and kindly feeling to withhold him from the task of pointing out to those who may profit, perhaps, the impossibility as well as the undesirability of further acceptance of shams for realities. If words mean anything, zoölogy and biology are not synonymous, and it is hoped that no false pride will prevent the zoölogists from

[1] Programme of Courses in Biology, 1892-93, p. 6. Chicago.

joining the botanists in the development of an accurate nomenclature. For while some sneer at nomenclature as a trivial matter and of no importance, it must be remembered that nomenclature is the expression of ideas, and ideas are of much importance.

CONWAY MACMILLAN.
University of Minnesota.

Photographs of Scientific Men.

A NOTE in your recent issue having to do with a request for the photographs of American botanists suggests that an appeal made through the columns of *Science* is likely to aid a collection made by myself. Some six or seven years ago, finding great difficulty in procuring the portraits of American scientists, I began gathering the photographs of the members of the National Academy of Science, and last year deposited in the Smithsonian Institution a collection of mounted portraits (with mounted autograph letters) of every member of our academy save two. This collection forms part, I believe, of the exhibit of the Smithsonian Institution at the Chicago Columbian Exhibition. The two portraits which are needed to make the set entirely complete are those of John Henry Alexander (1812–1867) of St. James College, Maryland, and later of the U. S. Coast Survey, and Jonathan Homer Lane (1819–1880), long connected with the U. S. Coast Survey and the U. S. Patent Office. I should be glad to obtain photographs of the two scientists or to make arrangements for the copying of any likeness of them known to exist.

MARCUS BENJAMIN.
640 Madison Avenue, New York City, May 18, 1893.

The Palæolithic Man in Ohio.

IN the second number of *The Journal of Geology*, Mr. Wm. H. Holmes has resumed his polemic against the evidence of the existence of palæolithic man in North America with a long article upon "Traces of Glacial Man in Ohio." Like his previous article upon the Trenton finds, this, too, is characterized by the kind of reasoning, which a correspondent of *Science* has called the argument *ad ignorantiam*, i. e., because he has failed to find palæolithic implements in a certain locality, therefore no one else has ever found them there. The present article, however, exhibits also a striking example of what might be called "the argument by monopoly." Mr. Holmes produces two fanciful cuts to show how the top of a gravel pit might have slid down so as to bury Indian relics coming from the surface; but he cannot see any sense in Professor Wright's preparing a plate to show precisely where in the same gravel-pit Mr. Mills actually found the object in dispute.

But the great difficulty about Mr. Holmes's discussion of this subject is that he has no correct appreciation of what a palæolithic implement really looks like. This is not to be wondered at when we reflect that his studies in "archæology" have been limited to investigations of the subject of "native art." He says "close analogies of form between Indian rejects and some varieties of European palæolithic objects are too common to permit the attachment of much value to this feature of this or any other similar find." Accordingly he proceeds to prepare a plate containing, besides the object discovered by Mr. Mills, of which he gives as good a copy as he can have made, four unfinished Indian celts found by him fifty miles away. Of these objects he says, "they correspond very closely in material and appearance with the New Comerstown specimen, as will be apparent from an examination of the plate. The figures are presented without identification in order that the student may, by an effort to distinguish them, convince himself of the similarity of the supposed paleolith to the quarry-shop rejects of the region."

Now I undertake to assert that any competent student of prehistoric archæology who has studied the subject in the Old World, where palæolithic implements have been found in large numbers, will have no difficulty in discriminating upon Mr. Holmes's plate between the true palæolithic implement and the four unfinished Indian celts placed beside it. All plates, however, fail to give a fair representation of solid objects like these, from the necessity of the case. They must be handled to be understood. The four unfinished celts resemble those previously figured by Mr. Holmes in describing the objects he discovered near Washington, where I have myself found similar objects several years ago. I repeat here, what I have said in another place, "no trained archæologist would hesitate for a moment to pronounce that the objects figured in the article entitled 'A Quarry Workshop' (*American Anthropologist*, Vol. III., plate 4) do not bear the slightest resemblance to real palæolithic implements."

I conclude this note with what I have already urged to the readers of *Science*, that "only a jury of the acknowledged prehistoric archæologists of the world is competent to pronounce judgment upon this question."

HENRY W. HAYNES.
Boston, May 13, 1893.

BOOK-REVIEWS.

Mineral Resources of the United States. 1891. By DAVID T. DAY. Washington, D. C., Department of the Interior, Government Printing Office. 1893. 630 p.

IT is somewhat unfortunate that these volumes cannot be more promptly produced, the late date of their issue impairing materially the value of the statistics contained. But in spite of this they are always welcome, and together—the present volume being the eighth in the series—they form a valuable component of every library. The arrangement is the same as in previous issues, and we find the familiar names of Birkinbine, Kirchoff, Weeks, Parker, and others under their respective specialties. Mr. Parker's statistical article on coal is exhaustive, occupying nearly 200 pages in all, and is supplemented by the articles on coke, petroleum, and natural gas by Mr. J. D. Weeks. Mr. Wm. C. Day continues his paper on stone from the "Resources" for 1889–90. An admirable and much-needed division appears upon the clay materials of the United States, written by Mr. Robert T Hill, and as this is in some respects the feature of the present volume an outline may not be out of place. Beginning with descriptive remarks, Mr. Hill passes on to the commercial classification, the origin and natural classification, residual or rock kaolins, and sedimentary or bedded clays. The sedimentary clays of the geological formations are given in natural sequence. The accessory minerals used in the clay industries are described and then the occurrence of clay materials by States.

Other interesting articles are those on natural and artificial cements, by Spencer B. Newberry, both descriptive and statistical, on precious stones by the expert, Mr. Geo. F. Kunz, and Mr. Packard's descriptive article on aluminum, the last including several pages on bauxite, with analyses and a sketch of the development in the South. Alabama, Georgia, and Arkansas are mentioned as containing the mineral, but Tennessee with its good promise, Virginia, and North and South Carolina are not spoken of.

An unfortunate slip of the binder has placed pages 49–64 inclusive between pages 32 and 33, but in other respects the book is all that can be wished for.

C. P.

William Gilbert of Colchester, On the Loadstone and Magnetic Bodies. A translation by P. Fleury Mottelay. New York, John Wiley & Sons.

A RATHER acrimonious discussion between Professor S. P. Thompson and Messrs. Wiley & Sons has attracted even more attention to this book than it would otherwise have received. It will be remembered that the Gilbert Club was formed in England a few years ago, and that one of the objects of their existence was the publication by subscription of Gilbert's works. Professor Thompson was one of the committee on publication, and the matter seems to have been left mostly to him. From various causes, one of which was possibly the fact that the latter is translating and editing a number of books on his own account, the publication of the Gilbert Club has been delayed. Previous to the determination of the club to undertake the publication of Gilbert's work, Mr. Mottelay had been seized with the same idea, and, as neither he nor his publishers were in any way infringing on the rights of the Gilbert Club, the work has recently been